

TESTIMONY BEFORE THE HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY
SUBCOMMITTEE ON SPACE AND AERONAUTICS

NASA'S SPACE SCIENCE PROGRAMS: REVIEW OF FISCAL 2008 BUDGET REQUEST
AND ISSUES

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Mr. Chairman, members of the subcommittee, thank you for inviting me here to testify today. My name is Lennard Fisk and I am the Thomas M. Donahue Distinguished University Professor of Space Science at the University of Michigan. I also served from 1987 to 1993 as the NASA Associate Administrator for Space Science and Applications. I appear here today in my capacity as the Chair of the National Research Council (NRC) Space Studies Board. The views I share with you today, however, are my own and not necessarily those of the NRC.

You have asked me to testify on the top three goals for NASA's Science Mission Directorate (SMD); the top three programmatic risks facing SMD; the top three strategic investments that should be made in SMD; and also to comment on the balance among the various science themes within SMD. The first three items are of course interrelated. The goals in part should be to eliminate the major risks, and identify the strategic investments needed to do so. I will thus answer these three questions as an interrelated set. I will then comment on the balance among NASA's space science disciplines.

Before considering the questions, I would like to comment on the recent history of SMD, since this context determines the goals, the risks, and the investments required. Throughout much of the history of the space program, space and Earth science in NASA was considered to be a fixed fraction of the NASA budget. In the mid-1990s, however, that rule was discarded, and the budget for space and Earth science was allowed to grow at the same rate as non-defense discretionary spending. Human space flight was not permitted this growth, and so the budget for space and Earth science became an increasingly larger fraction of the overall NASA budget. Whether deliberate or accidental, the result was that science in NASA was considered to be part of the Nation's investments in science, not simply as a fixed part of the investments in space. This rapid growth in science, however, was not uniform. The traditional space science disciplines – astrophysics, planetary sciences, and heliophysics – did very well. However, even in these times of growth in science funding, Earth science was kept at a constant budget, and then in FY2000 it began a steep decline in funding.

With the advent of the Vision for Exploration in FY2005, to extend human presence first to the Moon and then beyond, dramatic changes have occurred in the funding for SMD. Initially, the overall funding for space and Earth science, taken together, was projected to do well. Some

disciplines, favored in the Vision, did very well, in some cases at the expense of other disciplines; but summed together, the funding for space and Earth science continued to increase. However, it became increasingly obvious that NASA was not being provided with the funds required to execute the Vision; return the Shuttle to flight, and complete and use the International Space Station; maintain a healthy science program; and support its other missions such as aeronautics research. And so the squeeze was on. One by one, the funding for the various missions that NASA is responsible for have been reduced to a sub-optimum and, in some cases, critically inadequate funding level.

In the case of the funding for SMD, some \$3 billion was removed from the runout budget primarily to pay for the cost of the return to flight of the Shuttle and the completion of the International Space Station. There is no way to remove that much money from a budget without causing disruptions in ongoing programs and distortions in the balance among programs. Ongoing major flight programs, well into development, have priority; new flight programs – the future of the program – are seriously delayed or in effect cancelled. Small flight missions and basic research support – for technology development, the training of students, theory, data analysis, and new mission planning – all become vulnerable when there is a sudden and unanticipated change in the expected growth in funding.

To understand the inadequacies in the SMD budget, we need to consider how science is conducted. Science is about making discoveries – they can be profound discoveries that alter the concepts we hold of our place in the cosmos, or they can be minor discoveries that reveal some new aspect of a previously studied process. Discoveries lead to insight, insight to knowledge, and in some cases knowledge yields immediate applications that benefit society. Knowledge almost always benefits society in the long run.

A measure, then, of the health of a science discipline is the pace at which discoveries are being made. Similarly, the prospects for the future of a science discipline can be measured by whether there are any factors that limit the pace of discovery.

Space and Earth science is primarily an observational science. Our discoveries thus come from observations. In each of the disciplines in space and Earth science there are, in fact, extraordinary opportunities to make discoveries. Technology is advancing to where more detailed and revealing observations can be made. And our understanding of prior observations has improved to where we can search intelligently for new knowledge.

Given that abundant discoveries await us, if we are only bold enough to make the observations, the primary determinant of a bright future for space and Earth science is the rate at which we make new observations; that is, the rate of new space missions. And here the trends are very disturbing. For each of the disciplines in SMD there is a sobering downward trend in missions and thus opportunities for discovery. In the mid-1990s there was an average of 7 launches per year for missions in space and Earth science. In the last few years, the rate is more like 5 per year. In 2010-2012, the rate is projected to be under 2 per year.

There are some disciplines for which the downward trend in opportunities for discovery is clearly unacceptable. In Earth science, society is demanding to know the consequences of global

climate change in order to plan our future. In the other disciplines of space science, it is a grating waste of the nation's capabilities to reduce our pace of discovery. We have painstakingly built the infrastructure to make the nation foremost in the scientific exploration of space. To allow it to atrophy borders on neglect.

There is another consequence of the inadequacies of the SMD budget, and that is the vitality of our disciplines. The issue for space and Earth science is how do we ensure the infusion of new and better observing techniques, new minds, new ideas that challenge the established concepts? It is in fact very difficult to ensure the infusion of revolutionary technologies and concepts in budgets that are not growing. Rather, there needs to be new investments.

There is a need to maintain or, better yet, optimize the pace of discovery. There is a need to maintain the quality and vibrancy of the NASA science program through the introduction of revolutionary technologies and concepts. Both requirements demand a budget for space and Earth science that is growing. I remind you that the projected budget for space and Earth science in NASA grows at only 1% per year, which is a declining budget when inflation is included. There needs instead to be real growth.

Strategic Goals, Risks, and Investments for the Science Mission Directorate

The first strategic goal of the Science Mission Directorate (SMD) might well be stated – get back the money that was lost. A more constructive way to make this statement would be to note how inadequately NASA as an agency is currently funded. The agency is being asked to do too much with too little, and as a result all components of the agency, including science, are sub-optimally funded. We all need to recognize that without major relief to the total funding for NASA this nation does not have a viable space program capable of meeting the broad national needs that have been assigned to it. And we should all make it a strategic goal to provide NASA with the funding that is required.

The risk to SMD from inadequate funding is that it cannot perform its assigned tasks. The charge to the space and Earth science program in NASA is to explore the universe and lay down the foundational knowledge for the human expansion into space. It is to determine the future of the Earth, so sound policy decisions can be made to protect the future of our civilization. It is to contribute to the capability of the United States to compete in the world, whether it is through new knowledge, new technology, or a new workforce. The funding for space and Earth science in NASA, particularly the growth in funding in the years ahead is inadequate to perform this job, and failure to address this problem is a fundamental risk to the success of SMD in being able to fulfill its obligations to the scientific excellence of the nation.

The investment required in SMD is the same investment that the nation is prepared to make in the American Competitiveness Initiative. ACI has resulted in increases in funding for programs in fundamental science in, e.g., the National Science Foundation and the Office of Science in the Department of Energy. These programs were among only a few that saw increases beyond their FY2006 budget level in the enacted FY2007 budget. It is difficult, in fact, impossible, to distinguish between the fundamental science conducted by NASA in SMD and the fundamental science conducted by the NSF or the DoE Office of Science. It is interesting to note that had the

funding for SMD been allowed to increase in the same proportion as the NSF it would have followed the pattern of growth it had enjoyed in the late 1990s and the early 2000s, and would have provided funding that was better able to support the needs of the space and Earth science program.

The second strategic goal is for SMD to make more cost-effective use of the funds that have been provided to it. There is a disturbing upward trend in the cost of flight missions. The problem seems to be most egregious in the case of moderate and small flight missions. We seem unable to execute a mission of comparable complexity today for anywhere near the cost that was required in the previous decade. The cost of launch vehicles has increased. The cost of management oversight is increasing. We take actions that are perceived to reduce risk, but may not be cost effective. Whatever the reason, it should be a strategic goal to get the maximum science for the minimum funding, and, in my judgment, the most likely place to realize cost savings is in the execution of moderate and small flight missions.

There is a risk to SMD should it fail to improve the cost-effectiveness with which it executes moderate and small flight missions. Under any circumstance, funding will be limited. We need to get the maximum science for the minimum available funding, if for no other reason than to introduce flexibility into the SMD budget to fund new missions and needed investments.

Investments are required to achieve the strategic goal of improving the cost-effectiveness of small and moderate missions. Investments may be required in new launch vehicles so that the cost of access to space is reduced, particularly with the planned retirement of the Delta-II launch vehicle. Investments will be required in innovative management procedures and new technologies. There needs to be a concerted effort made to make full use of the best of the nation's vast infrastructure to conduct cost-effective space missions. We have great talent in this country for space hardware. We need to ensure that we are using this talent properly; that our processes ensure good engineering solutions and not simply someone's perceived reduction in risk.

If new funds for SMD can be provided, if missions can be executed more cost-effectively, or preferably both, the third strategic goal should be to use the funds realized to rebalance the program. When the funding in the out-years for SMD was reduced, the large flight programs under development were protected. It is the future that has been sacrificed. Missions still in technology development were halted. The pipeline that is essential to the development of technology and human capital – the Research and Analysis programs, sounding rockets, small flight missions – have been seriously disrupted. The portfolio of activities in SMD needs to be rebalanced so that we complete what we have begun, while at the same time we recognize that the scientific exploration and utilization of space is a long-term effort that will extend into the indefinite future. The investments that we make now, in people, in technology, in balloons and sounding rockets, in small flight missions, in the planning for future flight missions, will determine the vibrancy and the success of the scientific exploration and utilization of space in the decades ahead.

The risk of failing to meet the strategic goal of rebalancing the SMD program is, in my judgment, the most serious risk. The pipeline of human capital and technology has been

disrupted, and the future of the space and Earth science program is at risk. Consider a case in point. Almost every experimental space scientist currently practicing learned his/her trade in the sounding rocket or balloon programs. Yet with recent budget cuts, these programs are unable to perform this task. Small flight missions are the next step in the natural evolution of experimental capabilities, whether it is the development of new technology or the development of experienced scientists and engineers. And yet with recent budget cuts, the flight rate of small missions has been diminished compared to its previous rate.

It follows, then, given the importance of rebalancing the SMD program to protect the future of space and Earth science, that *an investment that ensures a proper pipeline in human capital and technology will have the highest return.* Research & Analysis funding, sounding rockets and balloons, and small flight missions all need to be restored to their proper place in the SMD program.

The Balance Among the Science Disciplines in the Science Mission Directorate

Each of the science disciplines in SMD – astrophysics, planetary sciences, heliophysics, and Earth science – has important tasks to perform, ranging from providing fundamental knowledge of the universe, to, in the case of Earth science, providing knowledge that is a direct and immediate benefit to society. Each of the disciplines has need of more funding, more cost-effective use of its funding, a rebalanced program, and the investments required to achieve these goals, as we discussed above.

In the case of Earth science, however, no amount of efficiencies, no internal rebalance within the discipline, no modest investment will provide the resources necessary. There is not adequate funding for Earth science in NASA to accomplish the mission that it has been assigned – to use the global vantage point of space to provide information on the immediate future of Earth, on which we can base sound policy decisions to protect our future. This deficiency is the result of a downward trend in the funding for Earth science that has persisted for a decade, and which has been in serious decline since FY2000. The recent NRC decadal survey for Earth science outlined the measurements and flight missions that NASA needs to accomplish, to provide society with the knowledge that is required. And the survey pointed out that these measurements can be made only if the Earth science budget, over the next several years, is increased back to at least the level of funding that was available in FY2000, an approximately \$500 million increase over the current budget.

This is not a rebalancing question, in the sense that Earth science should grow at the expense of other science disciplines. Nor should it grow at the expense of other programs within NASA. All of NASA's programs are currently inadequately funded. And all have a role to play in the national priorities. Rather, it is time for a new initiative, a specific directed task to NASA, with requisite funding provided, to pursue a vigorous Earth science program, in which the required measurements on the future of Earth are all made.

We need to consider NASA as an agency with many important tasks to perform. It is not just the agency that is to return us to the Moon, and all else is a secondary priority. Space is integral to the fabric of our society. We depend on it in our daily lives; we protect our nation through our

space assets; we use space to learn about our future; we enrich our society with knowledge of our place in the cosmos; we are moving our civilization into space; we expect the next generation of scientists and engineers to be versatile in the utilization and exploration of space. NASA has an essential role to play in each and every one of these national pursuits, and its role in each pursuit needs to be properly funded.

Thank you very much.

Biographical Information

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Lennard A. Fisk is the Thomas M. Donahue Distinguished University Professor of Space Science at the University of Michigan, where from 1993-2003 he was Chair of the Department of Atmospheric, Oceanic, and Space Sciences. Prior to joining the University in July 1993, Dr. Fisk was the Associate Administrator for Space Science and Applications of the National Aeronautics and Space Administration. In this position he was responsible for the planning and direction of all NASA programs concerned with space science and applications and for the institutional management of the Goddard Space Flight Center in Greenbelt, Maryland and the Jet Propulsion Laboratory in Pasadena, California.

Prior to becoming Associate Administrator in April 1987, Dr. Fisk served as Vice President for Research and Financial Affairs and Professor of Physics at the University of New Hampshire. In his administrative position, he was responsible for overseeing the University's research activities and was the chief financial officer of the University. Dr. Fisk joined the faculty of the Department of Physics at the University of New Hampshire in 1977, and founded the Solar-Terrestrial Theory Group in 1980. He was an astrophysicist at the NASA Goddard Space Flight Center from 1971 to 1977, and a National Academy of Sciences Postdoctoral Research Fellow at Goddard from 1969 to 1971.

Dr. Fisk is the author of more than 185 publications on energetic particle and plasma phenomena in space. He is a Member of the National Academy of Sciences (NAS) and the International Academy of Astronautics (IAA); he is a Foreign Member of Academia Europaea and a Fellow of the American Geophysical Union. He currently serves as Chair of the NAS Space Studies Board; he is a co-founder of the Michigan Aerospace Corporation and a Director of the Orbital Sciences Corporation. He is the recipient of the NASA Distinguished Service Medal in 1992, the AIAA Space Science Award in 1994, and the IAA Basic Science Award in 1997.

He is a graduate of Cornell University. In 1969, he received his doctorate degree in Applied Physics from the University of California, San Diego.